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Biogeosciences Discussions

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Interactive Comment

Interactive comment on "Nitrogen cycling in the Central Arabian Sea: a model study" *by* A. Beckmann and I. Hense

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We thank the reviewer for his/her comments. Unfortunately, many of them seem to be attributed to a lack of knowledge in theoretical and conceptual Earth System modelling and careful reading of the manuscript. Idealized process models are used widely in both physical oceanographic (e.g., Haidvogel et al. 1991, Beckmann et al. 2001, Xie and Vallis 2012) and in (coupled ocean-) biogeochemical modelling (e.g., Spall and Richards 2000, Follows et al. 2001, Hense et al. 2002, Parekh et al. 2005, Eugster and Gruber 2012). Since the reviewer seems unaware of this modelling philosophy, we decided to take the opportunity to once again clarify a few things.

(Reviewer comments in italics; our response in roman.)

My main criticisms can be summarized as follows:

1. *This is not clear how are derive the physical fields used to force the model. The authors use 2D mixing coefficients and advection velocities to force the biogeochemical model. From what is said, it seems like the authors pick up in different models (e.g; Miyama et al 2003; Lee 2004) physical fields without being sure that the obtained field is afterwards dynamically consistent. They seem to adapt the structure of this field in order to simulate gradients in the biogeochemical fields that agree with the data (shown Figure 1). We would expect that the adjusted fields are compared afterwards with some other modeling initiatives in order to check if the obtained fields are still acceptable. The authors may envisage that an inaccurate parameterization of some processes (too low rates) may also lead to a "bad" distribution of biogeochemical variables. What about the temperature? This is an important variable that affect biogeochemical processes and no information is given on how this field is estimated.*

   The physical fields are derived by abstraction, a normal practice for idealized (modelling) studies. The model of Miyama et al. (2003) shows a shallow overturning cell in the upper 100-150 m of the northern Indian Ocean, and a very weak circulation beneath. Hence, our circulation features such an overturning cell and much weaker flow beneath. Since the circulation beneath is less certain we tried several cases and found that a weak upwelling (consistent with the ideas of, e.g., Lee, 2004) is necessary to reproduce the nitrite distribution. Other flow fields we tested led to fundamentally different results (e.g., stronger flow removed the suboxic zone alltogether, due to the associated oxygen input from the south). The choice of the vertical mixing profile follows the general knowledge that mixing within the surface layer is large, that there is a stability-dependent minimum at its base and that the interior of the ocean is characterized by low diffusivities. So the flow and diffusivity fields are not tuned point by point to give the desired results, but rather employs the accumulated knowledge about the overturning circulation and stability dependent mixing. The prescribed circulation and diffusivity fields are dynamically consistent in the sense that no physical mechanism prevents the system from looking like this in the annual (and climatological) mean.

Concerning the statement that “an inaccurate parameterization of some processes (too low rates) may also lead to a “bad” distribution of biogeochemical variables“ we assume that the reviewer wants to say that wrong physical fields and wrong rates combined may fortuitously lead to realistically looking results: While this is possible in principle, in practice it is very unlikely. For a coupled and highly nonlinear system like this significant errors in the biogeochemical parameters cannot be easily compensated by a wrong flow field.

We have ignored temperature dependence, because temperature variability in the subsurface layer is small and organisms, particularly bacteria, are able to adapt to those conditions. Hence, our chosen rates represent a specific temperature environment. Please also note that one of the outcomes of the model is that individual processes occur in very limited depth ranges, so that the temperature-induced variations in bacterial rates are unlikely to be very large.

We like to conclude our answer by pointing out that a study like this does not attempt to say ”this is the only explanation of reality“ but rather ”we can explain certain aspects of reality with this limited number of assumptions“. This philosophy includes that another set of assumptions may lead to similar results. Of course any reader (or reviewer) may find some of our choices unsatisfactory and may look for other (more or less complex) approaches. But this does not automatically mean that nothing can be learned from this study.

2. *The hypothesis of considering an invariant physical field is a strong limitation and would deserve some more justifications. The authors consider that the seasonal time scales are not important for the subsurface distribution. This is an hypothesis that seems to be contradicted looking at Figure 1b where we see that the oxygen and nitrate profiles show gradients at 100m. This is not obvious that the position of the oxycline is not affected by the seasonal mixing. Moreover, even if the suboxic layer is less directly affected than the surface layer by the seasonal cycle, the particle flux export to the suboxic zone is seasonal and this will impact the suboxic layer.*

    Our justification for time invariant physical fields (and spatially invariant boundary conditions) is that we are interested in the time mean structure of the system. We think we have made this sufficiently clear in the manuscript. This is motivated by the observation that there are no strong seasonal variations in the suboxic zone (see, e.g., Sarma, 2002) and that the seasonal cycle of export flux in the eastern Arabian Sea, as shown for instance by Rixen et al. (2000), is small and not much affected by the monsoon.

    Unfortunately, there is no Figure 1b. If the reviewer is referring to the observed profiles in Figure 2 (center), the argument is still incomprehensible: How is it possible to extract statements about the seasonal variability from a snapshot?

3. *Even if the main focus of this manuscript is on the suboxic layer, the ability of the biogeochemical model to simulate the upper layer biogeochemistry is necessary since this is the flux of detritus that will drive the nitrogen and oxygen cycling of the suboxic layer. I would appreciate to see a convincing assessment of the performances of the biogeochemical model. We can deplore the lack of validation exercise performed in this work, the authors are satisfied that basic characteristics of the oxygen and nitrogen profiles are reproduced by the model*

BGD

9, C6866–C6879, 2012

Interactive Comment

and afterwards they use the tool to derive novel hypothesis. A variable that can be a good candidate to appraise model performances is N2 (adding a new state variable). I have some comments on the parameterization used (see my detailed comments).

As pointed out in our response to the first reviewer, the upper layer biogeochemistry *per se* is not that important as long as the export production is realistic (which is the case). In the framework of a theoretical study on the nitrite layer we could even exclude the surface mixed layer (SML) entirely and just prescribe the downward fluxes at the base of the SML.

We disagree about "adding a new state variable (N_2)". What could be gained by adding yet another quantity which is produced at unknown rates? This request is also a contradiction to later statements of the reviewer, who criticizes that we have too many unconstrained parameters.

To summarize, there are too many degrees of freedom in this study: the physical fields are badly constrained and are adapted to reproduce the oxygen and nitrogen profiles although this is not sure that the misrepresentation of these profiles is due to a "bad" physics, parameters used to express processes like DNRN, DNRA, N2RN, ANAMMOX, A-DENIT are also poorly constrained, the biogeochemical model is poorly validated especially in the upper layer, the influence of lateral boundary conditions on the quality of model results may be important since data profiles are imposed, the imposition of a surface and lateral flux of nitrogen and oxygen for which very few information are available (the surface conditions for oxygen is notably very crude and this is not clear how nitrogen fixation is parameterized, this process is mentioned in the text but does not appear in the equations). Due to these large number of poorly constrained processes and the lack of a thorough validation exercise I do not think that this model can be used afterwards for diagnostic purposes.

Again, we are puzzled by the reviewer's assumption that wrong rates can "easily” be cured by an equally wrong circulation and that our good agreement with observations is just by accident (see also our comment above).

Concerning the surface layer biogeochemistry, we refer to our response to the first reviewer of this manuscript. The only relevant issue is that export production is quantitatively correct and this is the case (see also our comment above).

We also contest the assertion that our boundary condition for oxygen are inadequate. In the framework of idealized process studies it is well established to use such simplified boundary conditions.

Concerning nitrogen fixation, the reviewer seems to have skipped reading the section “model configuration”, where we explained how this process is included.

So obviously, we also disagree with the concluding statement that “this model cannot be used for diagnostic purposes”. We have not used any *ad hoc* assumptions neither for circulation patterns nor for the biogeochemical part. The results obtained were not clear *a priori*. We show that, for our choice of parameters, we are able to produce realistic phenomenology, and realistic rates for several bacterial processes in the suboxic zone; we offer explanations for observed differences in nitrite vertical structure that do not even exist in other models, and we point out that previous estimates may be too high due to assumptions that may be incorrect. Our conclusions are not intended to be the final word on the issue, but to spur new investigations that may eventually improve our understanding of the system.

Detailed comments

(The page numbers used by the reviewer are misleading. Please note that “page 2” refers to page 13582 but “page 3” and all following refer to page 13584 ff.)

- *Section 1, page 2 Line 4: incomplete sentence*

Sorry, we forgot a comma before the last word. It should be “Under low oxygen conditions the balance between the reactive nitrogen compounds, i.e., ammonium, nitrite and nitrate, changes.”

- *page 2 Line 9-10: please add a reference after system*

  (actually, this comment refers to lines 25/26) Since we go into much more detail later on in this matter where we list all references, we find this unnecessary.

- *page 2 Line 10: What do you mean by “in both denitrification process”?*

  (this seems to refer to line 26) If the introduction is carefully read it should be obvious that we mean autotrophic and heterotrophic denitrification.

- *General comment on page 2 and 3: This is sometimes confusing to understand the exact process to which the authors are referring. For instance, is annamox not a denitrification process? The authors list the processes that may occur in suboxic conditions affecting the nitrate and nitrite contents. All the processes listed between lines 5-13 lead to a loss of nitrate or nitrite. Can not they all be considered as denitrification processes? For instance, line 12-13, the authors mention autotrophic anammox as an example of denitrification (in both denitrification processes ... as mentioned at line 10). Please give some clarifications.*

  It seems that the reviewer is not overly familiar with the multitude of bacterial processes involved in the nitrogen cycling in the suboxic environments. DNRN and DNRA lead to a loss of nitrate and nitrite but not to a loss of nitrogen from the system! Referring to these processes as denitrification would be highly misleading. Additionally, anammox leads to the loss of nitrogen but is not a denitrification process. Carefully reading the introduction, a close look at the flow diagram and the stoichiometric equations in the Appendix should help to understand our terminology and the rationale behind it.

- *Page 2, Line 20 DNRN has already been defined.*

  (this is actually page 3, line 10) We will remove the repeated definition.

- *Page 3 Line 6: anammox is also ammonium oxidation.*

  It is unclear what the reviewer means with this comment. We wrote "In addition, the method does not permit the identification of individual processes (anammox, denitrification, DNRN, DNRA) involved in N-cycling." Maybe again a confusion with respect to page and line numbers?

- *Page 6: please give some details on how the atmospheric inputs and lateral inputs are imposed and how they have been estimated. Besides, how are model results sensitive to the values imposed? The values imposed will probably strongly conditioned the amount loss by denitrification and export processes. Therefore, the way they are constrained is essential. This is a crucial point that the authors need to address.*

  The requested information is all given in section 3.3 "Model configuration". As pointed out in the manuscript we consider idealized conditions, without spatial or temporal variability. We do this on purpose to model a system with as little complexity as possible. Even the reviewer mentioned the many degrees of freedom (see above). Therefore we have reduced the degrees of freedom by not considering meridional variations in light, mixed layer thickness, nitrate and oxygen concentrations in 1500 m depth, as well as patchiness of biological fields and more. The goal of a process study is not to simulate a particular situation in the Arabian Sea but to learn about the system.

- *Page 6 Lines 5-9: The authors mention that "they exclude as many complicating aspects of the physical environment as long as the main phenomenology is capture and general quantitative agreement is obtained". This is not a trivial task to identify what are the main processes that will influence the dynamics of the suboxic zone. It may require to start first with a complex framework and performing sensitivity studies in order to appraise what are the driving mechanisms. I would like that the authors clarify how they deal with this complex question.*

  In our opinion the approach is straightforward: We start with a simple configuration and see, if the results reproduce observed fields and rates, if not, we add complexity until they do.

  Please note that this is just like in observational work. Researchers start by observing a number of quantities, and if these can be successfully used to explain a certain phenomenon, there is no need to observe additional variables. Only if there is something that cannot be explained, one has to dig deeper. This is how science works. We never said that we can explain everything. But we say that we can reproduce some of the observed phenomena with our model.

- *Besides, it would be helpful that they clarify which complicating aspects they ignored, what are the main phenomenologies that need to be captured ...*

  Our model ignores: seasonality, lateral advection in the third dimension, interannual and interdecadal variations, temperature dependence of rates, variations in vertical mixing, ... The phenomenologies are: the deep profiles in the core and the shallow ones in the edge region, along with the typical rates for various processes.

BGD

9, C6866–C6879, 2012

All of these things are mentioned in the text. So if they are not clear to the reviewer, it can only mean one thing: the manuscript has not been read carefully enough.

- *page 6 Line 7: I would suppress the first sentence since it is repeated afterwards*

  We will remove the sentence in line 7 on page 8, instead.

- *Page 7, lines 6-8: it is not clear why the oxygen budget is not balanced in the model. Could you please show a scheme with the oxygen flows (including air-sea interactions) and transports?*

  The flow of oxygen is depicted in Figure 3. Again, we are puzzled by this comment. We are not aware of any model that explicitly includes oxygen in water molecules in the balance equations. Instead, it is common practice in marine biogeochemical models to assume that oxygen is produced by photosynthesis "out of nothing", as there is an unlimited source of water. We believe this can be considered basic knowledge.

- *Page 7, Line 28: this is a very subjective choice that would deserve some comparisons with other modeling work considering the importance this choice may have on the solution.*

  We have no idea what the reviewer means. We wrote "Proper description of specific processes like anammox requires the determination of all participating bacteria and the derivation of a suitably weighted average of metabolic rates and half saturation constants." Maybe a confusion with respect to page and line numbers again?

- *Page 8 Line 1: please add a reference*

BGD

9, C6866–C6879, 2012

Interactive Comment

see answer to the next question

- *Page 8 Line 2: Please add a reference*

  There are several studies evaluating the growth rates of oceanic phytoplanktonic and bacterioplanktonic organisms. Textbooks about marine microbial ecology (e.g., the new one by Kirchman, 2012) report that the growth rates of photoautotrophic organisms are higher than those for heterotrophic bacteria and that the growth rate of anaerobic autotrophic bacteria are significantly lower than of aerobic autotrophic organisms. Of course, the actual bacterial turnover rates will depend on the composition of detritus (i.e. the fractionation between labile and refractory). Labile detritus can be decomposed by heterotrophic bacteria under aerobic and anaerobic conditions at similar rates (Kristensen et al., 1995). We agree that some references could be provided (although it seems rather unusual to refer to textbook knowledge).

- *Page 8 Line 8: do you mean an inhibition by oxygen?*

  It is unclear what is meant here. We may speculate that the reviewer means our statement in lines 11/12: then, yes, we mean that we assume an inhibition by oxygen for anaerobic processes.

- *Equation for oxygen: I suggest to use parameters instead of directly 0.5 and 1.5 as done with the other terms.*

  This will be done.

- *Page 9 Line 10: why the authors are not using a classic monod function and 1-Monod to describe the limitation and inhibition by oxygen? The chosen formulation makes the mode really dependent on the value selected for “Theta”.*

BGD

9, C6866–C6879, 2012

We found it necessary to use a threshold value, above which all suboxic processes cease to occur. This way, suboxic and oxic zones (with their different bacterial communities) are strictly separated (we will point this out in the revised version of the manuscript). Besides, even for a Monod function, the “half-saturation constant” would be a similarly important parameter.

- *The surface boundary condition for oxygen is very crude. Normally the flux is computed from a saturation concentration*

  There is nothing particularly crude about our boundary conditions in the framework of this process study. (If the SML concentration his higher than the prescribed atmospheric value, there is out-gassing, otherwise, there is an input of oxygen.) As pointed out above: If we do not get the right phenomenology, we might consider adding a more realistic surface boundary condition for oxygen (although that would not be highest on our list). But since the results are in reasonably good agreement with observations, there is no need for it at this point.

- *Page 10 Line 20: the authors consider that the seasonal time scales are not important for the subsurface distribution. This is an hypothesis that seems to be contradicted looking at Figure 1b where we see that the oxygen and nitrate profiles show gradients at 100m. This is not obvious that the position of the oxycline is not affected by the seasonal mixing.*

  Please see our comments under 2. above.

- *Page 11 Line 9-11: This is not clear what was the physical model that provide the physical fields to force the biogeochemical model. From what is said, it seems like the authors pick up in different models (e.g; Miyama et al 2003; Lee 2004)*

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Please see our comments under 1. above.

- *Page 11 Line 22: this is not clear how nitrogen fixation is modeled in this work.*

Please see our comment under “To summarize ...” above.

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